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| $\frac{D}{d}$ | T Absolute  |             | Newton<br>$T_0 = 300^\circ$ |
|---------------|-------------|-------------|-----------------------------|
|               | Newton      | Stefan      |                             |
| 1             | $0.7^\circ$ | $300^\circ$ | $300.7^\circ$               |
| 2             | 2.8         | 424         | 302.8                       |
| 3             | 6.3         | 520         | 306.3                       |
| 4             | 11.2        | 600         | 311.2                       |
| 10            | 70          | 949         | 370                         |
| 20            | 280         | 1,342       | 580                         |
| 30            | 630         | 1,643       | 930                         |
| 40            | 1,120       | 1,897       | 1,420                       |
| 53.4          | 2,000       | 2,192       | 2,300                       |

Considering now, for example, the temperatures corresponding to the values 1 and 2 for  $D/d$ , the total failure of Stefan's law is at once apparent, for while the increase of temperature corresponding to an increase in the aperture from  $D = 0.837$  in. to  $D = 0.674$  in. is consistently  $2^\circ.1$  C. according to Newton's law, the increase according to Stefan's laws is  $124^\circ.0$  C., an absurd result! Again, as the other waves direct from the sun enter and reach the bottom of the earth's atmosphere the focal temperature due to these *direct* waves must evidently be measured from a totally different starting-point. If the absolute temperature of air at the place of observation is  $T_0$ , then  $T_0$  must be taken as the origin from which the temperatures, properly belonging to the *direct* solar radiations alone, must be measured. If for the present case we have  $T_0 = 300^\circ$  C., the theoretical values given in the fourth column will result from Newton's law. Now when we come to apply this same line of reasoning to Stefan's law, the data given in the third column become both absurd and unintelligible for ordinary temperatures (corresponding to small values of  $D$ )!

Much of the confusion heretofore existing regarding the temperature of space can, in my opinion, also be traced to the largely prevalent but mistaken idea that the ordinary mercurial thermometer is a suitable instrument for measuring *direct* radiations, when in fact this thermometer then simply measures the stored-up energy trapped in the "hot-house"-like form of this particular instrument. The ideal thermometer will be one which gives instantaneous results, since the intensity of the

ether vibration is independent of the time. For this reason the platinum plate in my observations was hammered so thin that the evidence of melting was secured from practically instantaneous exposures in the solar focus. It will be noticed that for the value 53.4, corresponding to  $D = 18$  inches, I have assumed the actually measured focal temperature to be  $2,300^\circ$  C.; the excess over the accepted value for the temperature of melting platinum I have roughly estimated to be equal to the losses resulting from causes similar to those which Professor Very mentions in the second paragraph of his paper.

In any case, I hold that however great the possible error of my measured value for the focal temperature may be, this error can not affect the validity of my theoretical formulas.

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ANN ARBOR, MICH.,  
February 17, 1908

# SPECIAL ARTICLES

## THE HEREDITY OF SEX

In *Proceedings of the Zoological Society*, 1906, I., p. 125, Doncaster and Raynor described certain remarkable experiments respecting the inheritance of the moth *Abraxas grossulariata* and its variety *lacticolor*. This variety was originally known in the female form only. Experimental crossings showed the following results:

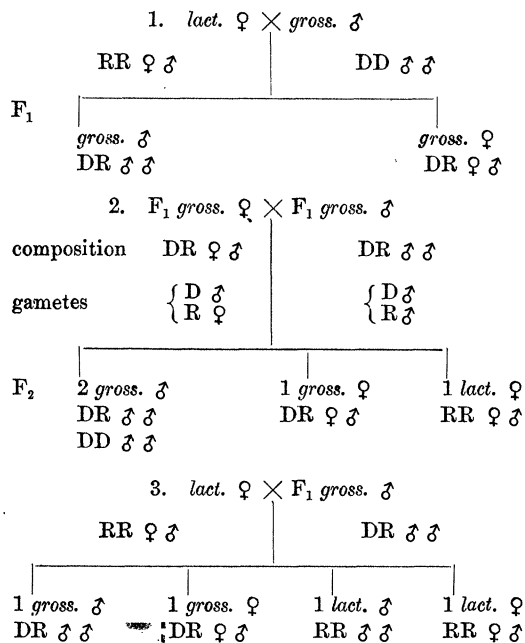
1. *Lact.* ♀ × *gross.* ♂ gave  $F_1$  ♂s and ♀s all *gross.*
2.  $F_1$  *gross.* ♀ ×  $F_1$  *gross.* ♂ gave *gross.* ♂s, *gross.* ♀s and *lact.* ♀s; no *lact.* ♂ being formed.
3. *Lact.* ♀ ×  $F_1$  *gross.* ♂ gave all four possible forms, *gross.* ♂s, *gross.* ♀s, *lact.* ♂s, *lact.* ♀s. The ♂ *lacticolor* thus raised were the first that had ever been seen.
4.  $F_1$  *gross.* ♀ × *lact.* ♂ gave all ♂s *gross.* and all ♀s *lact.*

In discussing this curious series of facts Doncaster adopted Castle's view that each sex was heterozygous in sex, and that each gives off male-bearing and female-bearing gametes. He then shows that if it be assumed (1) that in the  $F_1$  ♀ there is coupling such that the male ova all bear the *grossulariata* factor and the female ova all bear *lacticolor*; (2) that in

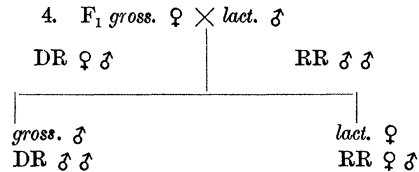
the gametes of the male there is no coupling; (3) that in fertilization union can only take place between gametes bearing opposite sexes; (4) that dominance attaches to the sex brought in by the ovum; the observed results would be produced.

On reexamination of the case we have found a much simpler and, as we think, more probable account of the phenomena. Two assumptions only are needed: (1) that one female is heterozygous for sex, femaleness being dominant, and the male a homozygous recessive; (2) that when in  $F_1$  the two dominants, femaleness and the *grossulariata* factor coexist, there is a repulsion between them, such that each gamete takes one or other of these two factors, not both. Such mutual repulsion of two dominants has already been shown to occur in the sweet pea when a plant is heterozygous for the upright standard and for the blue factor, constituting what must be regarded as a spurious allelomorphism between them.<sup>1</sup>

The whole series of facts is then consistently represented as follows, D and R representing the presence and absence of the *grossulariata* factor:



<sup>1</sup> SCIENCE, N. S., XXVI., p. 655.



The numbers originally obtained were very wild and irregular, but on repeating the experiments Doncaster got results which are numerically very close to expectation.<sup>2</sup>

Attention is drawn to this case as illustrating the great value of evidence respecting dissimilarity in reciprocal crosses for the elucidation of the problem of sex-heredity. We have long been engaged on a more complex case of this kind, the heredity of the black pigmentation of the Silky fowl, in its crosses with brown Leghorns and other fowls with light shanks. The facts there also point very clearly to a similar solution, but it may be some years before the evidence is complete.

On general grounds it seems to us probable that one and not both sexes of the same organism will be shown to be heterozygous for sex, and that the approximately equal output of the two sexes in ordinary cases is a consequence of this. Correns<sup>3</sup> has lately shown that his experiments with *Bryonia* suggest that in that plant it is the male which is heterozygous. Russo<sup>4</sup> from his experiments

<sup>2</sup> *Nature*, 1907, LXXVI., p. 248.

<sup>3</sup> *Bestimmung und Vererbung des Geschlechtes*, Borntraeger, 1907.

<sup>4</sup> *Atti Ac. Lincei*, 1907. Heape (*Proc. Roy. Soc.*, Vol. 76, B, 1905) described in the rabbit processes by which ovarian ova frequently degenerate, apparently as a normal occurrence. Mr. Heape very kindly gave us an opportunity of examining his preparations, and it was impossible to avoid being impressed with the general resemblance which such degenerating ova bore to those which Russo regards as destined to become males. Consequently before that view of their nature is adopted, the relation of the so-called "male" ova to the degenerating ova will need very careful study; for it seems as yet not unlikely that those differences which Russo has taken to indicate maleness may prove to be due to incipient degeneration. Also as regards the important question of the alleged effect of lecithin in increasing

on rabbits concludes that sex is determined by the ova, which he regards as male and female, respectively, or in Mendelian terminology, that it is the female which is heterozygous.

There is, we think, no reason *a priori* why in nature generally dominance should be the special property of one sex alone. We rather anticipate that dissimilarity will be found between the great groups in this respect.

Consistently with the view that in Vertebrata the female is heterozygous, the production of male secondary sexual characters ensues in the female consequent on ovarian disease, while castration of the male produces effects which may perhaps all be regarded as arrests of development. In the Crustacea, however, the work of Geoffrey Smith and of Potts on parasitic castration points to the converse conclusion, namely, that the male is there heterozygous for sex, assuming definite female characters when castrated, while in the female castration merely arrests development.

Correns refers to E. B. Wilson's facts respecting the accessory chromosome as supporting the view that the male is the heterozygous sex, and we have lately done the same.<sup>5</sup> Doncaster, however, has pointed out to us what must be a serious difficulty in the application of this argument; for if the male sex be dominant, it has then to be supposed that dominance attaches not to the presence of the accessory chromosome, but to its absence, since it is in the female that the accessory chromosome is paired. Great weight we think must be given to this criticism. Dominance, as we now suppose, is due to the *presence* of something which is *absent* from the recessive, and we are almost precluded from imagining that the absence of a chromosome can be a cause of the dominant quality.

In order to bring the facts of sex inheritance in the parthenogenetic forms (bee, aphid)

the output of females, an opinion can scarcely be formed on the cases published by Russo, for these are declared to have been selected. It is to be hoped that the full statistics will soon be published.

<sup>5</sup> SCIENCE, XXVI., 1907, p. 658.

into line with our view, it would perhaps have to be supposed that sex segregation in these types takes place not between gametes, but between the primitive soma and the germ plasm, so that the ova would all bear the recessive male character and the spermatozoa the dominant female factor. To discuss this suggestion in detail would, however, carry us beyond the scope of this note.

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W. BATESON

CAMBRIDGE, ENGLAND,  
March 19, 1908

#### PRE-CAMBRIAN ROCKS IN SOUTHEASTERN WYOMING<sup>1</sup>

DURING the past summer the ancient rocks of the Laramie and Sherman quadrangles in southeastern Wyoming were studied in some detail. The maps cover a portion of the Laramie Mountains and the easternmost spurs of the Medicine Bow range. It appears that most of the region is underlain by a coarse-grained red granite, but there are scattered patches of older rocks which show various degrees of metamorphism and bear complex relations to one another.

The oldest rocks recognized in the district are a series of schists and gneisses, which are largely metamorphosed volcanics, although they contain some rocks clearly of sedimentary origin, and others which are doubtful. The supposed volcanics include hornblende schists and schistose rhyolites. Some occur in the form of dikes, while breccias indicative of surface extrusives were recognized in several places. Certain highly quartzose rocks and tremolite-schist are interpreted as altered sediments. The rocks are so highly folded, metamorphosed and cut by later intrusions that the relations of the different members to each other are very obscure and have not yet been elaborated.

Next in age follows a group of granitic gneisses, which are evidently metamorphosed granites. They are clearly intruded into the schists just mentioned. There are at least two distinct varieties of these gneisses: one

<sup>1</sup> Published by permission of the Director of the U. S. Geological Survey.